kinds of blows were intermingled irregularly, and according to a list previously drawn up, and the damper was held up while the note was being struck. As each note was struck a verdict as to its quality was given by a person sitting out of view of the piano, and this verdict was compared with the character of the blow given. The observer, it should be mentioned, was not a trained musician, but was exceptionally appreciative of what is called touch.

Discarding all cases where the note had accidentally been more or less loud than the average, it was found that in 49 per cent. of the cases of uniform loudness the effect on the auditor coincided with the character of the blow given, in 51 per cent. the reverse. In another series of 100 notes the values were 51 and 49 respectively. It is evident, therefore, according to this, that different quality of touch produces no difference in the quality of the sound produced. No doubt further evidence should be obtained by a repetition of such experiments with other performers and other observers, and a further test would be obtained by having two performers alternately striking the same note, and ascertaining whether the observer could detect the difference.

Even if the independence of "touch" on the character of the stroke is fully established, there is no difficulty in explaining the apparent difference in touch of a performance with the fingers and one with a piano-player. At least seven factors may be specified as determining touch:—(1) Differences in loudness of a note (or notes) as compared with that of the preceding notes, (2) deviation from the theoretical value, according to the score, of the duration of the note as compared with that adopted for the preceding and succeeding notes (*i.e.* different degrees of *staccato* or *legato*); (3) variations in time as compared with that of the preceding notes; (4, 5, and 6) similar differences in loudness, duration, and time of one note in reference to its nominally contemporary notes; (7) the use of the loud pedal. The use of the soft pedal is so specialised that it need not be considered; it is used more for modifying a whole passage than for modifying individual notes, and it practically gives the performer the command of a second instrument; but, perhaps, an eighth factor of a general character should be included, namely the extent to which the above means of emphasising notes is made to harmonise with the rhythm and sense of the music.

Now, in the piano-player we have the means, either by moving levers or by pressure on the bellows, of altering the loudness (1) or time (3) of a note as compared with that of preceding notes, but such alterations are gross and sluggish in comparison with those possible in finger playing; the inertia of the mechanism has to be overcome, and the result depends directly or indirectly on the pressure of air in the bellows, which cannot be altered instantaneously. The least unsatisfactory operation is a retardation of the time. The duration of the note as compared with that of its neighbours (2), or of its fellow-notes (5), as well as the loudness of it as compared with its fellow-notes (4), can only be accomplished by modifications in the slots of the roll, and they are, therefore, stereotyped and always the same; this produces a very unsatisfactory result.

When hearing for the first time a well-played piece with the air strongly brought out in this way, the impression produced is decidedly favourable, but at the second hearing a sense of irritation is felt; we know exactly the degree of emphasis which is going to be placed on each note of the air, and the monotony is even more trying than where no emphasis is attempted. It is a case similar to that of a beautiful landscape which never changes; the charm of music lies largely in its imperfections, or, at least, in the varying and unexpected degree in which its beauties are brought out. An alteration in the time of contemporaneous notes (6) is, I believe, attempted on some rolls, but the effect, I should imagine, would be even less satisfactory than in the other cases. The use of the loud pedal is a potent defect in the piano-player. Needless to say, this pedal is not used in good finger playing only to produce loudness, but more generally to produce softness, and a smooth flow of sound. In a slow movement a good performer will often depress and raise the pedal for nearly every note, and the effect produced depends entirely on the correct timing of these movements with the depression of the keys; this is impossible unless the messages from the brain to the fingers and to the feet are simultaneous. This cannot be so with a piano-player, where the sound is produced by a separate mechanism; this sound (either of the particular note in question or of its predecessor) must travel to the brain, which then has to interpret it, and to send a message to the finger which controls the pedal lever; the performer is conscious of an act of thought being necessary in using the pedal of a player, whereas in finger playing its use appears as if it were instinctive. With the player it is practically only used for producing loudness.

I believe that the thud of the air on the keys is another defect in the player. When in the same room as the instrument, it seems possible to decide after hearing one bar whether the player or fingers are being used, and this is certainly so, as I have ascertained by trial, and apparently more easily so, when one is in a distant room, the reason of this being that the thud penetrates the walls more easily than the note, and hence attains more relative predominance. I have an instance of this in a striking clock, of which the note is inaudible in the next room, while the thud can be distinctly heard.

No doubt many of the defects of the piano-player will be diminished in time, especially by such devices as those of Prof. Bryan. Already one of the best piano-makers is putting on the market an instrument which is a great advance on its predecessors, the chief feature of it being a reduction in the size of the bellows, which admits of much greater control over the sound production. Still, it is a case of playing with the feet, instead of with ten independent fingers.

When the damper is allowed to act in the ordinary way, it is possible that the effect produced (touch) may be modified by the character of the blow given to the keys, for this blow results in the damper being raised, as well as in the hammer striking the strings, and these two actions may not synchronise to the same extent with blows of different character.

SPENCER PICKERING.

A Danger of so-called "Automatic Stability."

FROM time to time devices have been proposed for securing "automatic stability" in aëroplanes by means of a suspended weight or "pendulum," which operates on rudder-planes governing the motion of the machine. A similar device is also in actual use for governing the motion of torpedoes in a vertical plane, in conjunction with a further device for maintaining the torpedoes at a constant depth below the surface, or more strictly at a level where the hydrostatic pressure is constant. "Pendulum" arrangements for automatic stability of aëroplanes have frequently figured in the pages of such journals as *The Scientific American*, and it must be admitted that such devices are calculated to appeal strongly to the imagination of readers whose knowledge of dynamical principles is limited in range or *nil*.

The statement, which stands in my name, to the

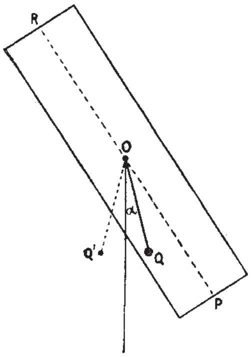
© 1913 Nature Publishing Group

effect that such a device increases the number of degrees of freedom of the apparatus with an accompanying increase in the number of possible oscillations and of conditions necessary for stability is, I believe, incontrovertible. One form of dynamical instability that may result in such cases is the setting up of violent oscillations, ever increasing in amplitude, in the pendulum itself, accompanied by flapping of the control planes, in which case this particular method of control becomes worse than useless.

The remedy which naturally suggests itself, in such circumstances, is to damp down the oscillations of the pendulum by means of frictional or other resistances, and it is probable that few university graduates who have taken first-class honours in mathematics would think that such a contrivance could possibly be wrong. The following test case will show how very dangerous it is to attempt to draw conclusions from general considerations.

For the aëroplane or torpedo, we substitute a heavy, rigid body POR, free to rotate without resistance about a horizontal axis through its centre of gravity O, perpendicular to the plane of the paper, and therefore, in the absence of other causes, in neutral equilibrium, and we assume that the moment of inertia of this body is considerable.

We next imagine a light, small pendulum OQ to be fixed in bearings in the body POR, so that it can turn about the same axis, but we suppose that a frictional couple is called into play between the large body and the pendulum at these bearings. The pendulum being light, this frictional couple exerts no appreciable effect on the large body POR, but the friction is sufficient rapidly to damp out the oscillations of the pendulum itself. The effect of a rudder plane controlled by the pendulum we represent by the assumption that the pendulum operates some mechanism which impresses on the large body a couple proportional to the angle QOP, tending to make it revolve towards OQ, the object of this couple being to bring that body into a position of rest in which OP is pointing vertically downwards.



When the large body is rotating in the counter-clockwise direction (as in the figure) the small pendulum assumes a position of equilibrium OQ on the right-hand side of the vertical, and inclined to the vertical at a certain angle α, the moment of its weight then just balancing the frictional couple. When the body begins to swing backwards the pendulum swings with it until both have described an angle 2α, so that the pendulum occupies the position OQ′, now making an angle α on the opposite side of the vertical. During this portion of the motion the controlling mechanism impresses on the body a constant angular acceleration, because the angle QOP remains constant. Consequently in the new position the body is rotating with a certain angular velocity set up by this acceleration. In the subsequent motion the pendulum remains at rest in the position OQ′, and the body performs a simple harmonic rotation about OQ′, but owing to its initial angular velocity it does not come to rest until its angular distance from OQ′ is greater than the angle QOP. It follows by this reasoning that the oscillations increase in amplitude, and this effect owes its existence to the frictional couple.

G. H. BRYAN.

The Structure of the Diamond.

WE have applied the new methods of investigation involving the use of X-rays to the case of the diamond, and have arrived at a result which seems of considerable interest. The structure is extremely simple. Every carbon atom has four neighbours at equal distances from it, and in directions symmetrically related to each other. The directions are perpendicular to the four cleavage or (111) planes of the diamond; parallel, therefore, to the four lines which join the centre of a given regular tetrahedron to the four corners. The elements of the whole structure are four directions and one length, the latter being, in fact, $1{\cdot}52 \times 10^{-8}$ cm. There is no acute angle in the figure. These facts supply enough information for the construction of a model which is easier to understand than a written description.

If we proceed from any atom, using only standard directions, to the next but one, the straight line joining the first to the last is a diagonal of a face of the cubical element of structure; if we move in the same way through four stages, using all four standard directions in turn, the straight line joining the first and the last is a cube edge. Starting from any atom we can return to it after six stages, using three standard directions twice each. In this way we always link together rings of six carbon atoms.

If the structure is looked at along a cleavage plane it is seen that the atoms are arranged in parallel planes containing equal numbers of atoms, but separated by distances which alternate and are in the ratio 3 : 1 (actually $1{\cdot}52 \times 10^{-8}$ cm. and $0{\cdot}51 \times 10^{-8}$ cm.). It is a consequence of this arrangement that no second order spectrum is reflected by the (111) planes, although spectra of the first, third, fourth, and fifth orders are found. It was this fact that suggested the structure described above. Several other tests, however, may be applied, and all are satisfied.

Zincblende appears to have the same structure, but the (111) planes contain alternately only zinc and only sulphur atoms. In this way the crystal acquires polarity and becomes hemihedral.

W. H. BRAGG.
W. L. BRAGG.

Leeds, July 28.

Artificial Hiss.

REPLYING to the inquiry of Lord Rayleigh (in NATURE of May 29, vol. xci., p. 319) as to the way in which an artificial hiss may be produced with a moderate pressure of air, I suggest that a current of air directed against a sharp edge of a knife held somewhat obliquely may answer his purpose.

In this connection it is interesting to note that for the formation of the hissing sound in our mouth the presence of saliva seems necessary. If I dry the tongue and the other parts which are needed for the pronunciation of the hissing "s," it is almost impossible to produce an audible "s," and the tongue—instinctively, as it were—makes an effort to gather some saliva and to wet itself.

I would therefore suggest that Lord Rayleigh wet the end of the rubber tube with which he experimented.

FRED J. HILLIG.

Kioicho 7, Kojimachi, Tokyo, July 1.

IT had occurred to me also that the moisture of the mouth might play a part in the production of a hiss, but I do not find that such drying as I can give makes an important difference.

I have to thank several correspondents for suggestions. In particular, Mr. G. Beilby sent me two pipes suitable for a 4 in. water pressure, which gave a better effect than anything I had then tried, but still, in my estimation, much short of a well-developed

© 1913 Nature Publishing Group